

SATURDAY, MAY 30, 1874.

ORIGINAL LECTURES.

CLINICAL LECTURE

ON A CASE OF CEREBRAL RHEUMATISM, SO CALLED.—USE OF COLD BATH.

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GENTLEMEN,—A few days since, upon entering the ward my attention was instantly attracted by the expression upon the face of a patient. He was a young, temperate Irishman, twenty years of age, and of vigorous physique, who had passed through a severe attack of inflammatory rheumatism without cardiac complications, and was suffering from a relapse, which first appeared as a subacute inflammation of the knee. I had not seen the man the previous day, but I find in the note-book of my very able resident, Dr. Bruen, the following:

"Second day of relapse. This morning an acute inflammation of the wrist-joints has set in; the fever is very high; temperature in the axilla 104° Fahr.; ordered potassii bicarb. gr. xx every two hours."

As we walked to the bed, in reply to a question, "What ails our rheumatism patient?" Dr. Bruen said, "Nothing, unless it be pericarditis. When I saw him at 10.30 A.M. there was much less inflammation of the joints than the preceding morning, and although his temperature was as it had been, 104° Fahr., and, as I thought, a pericardial friction-sound could be heard, yet the man was doing fairly: perfectly rational, with a good pulse." It was now about half-past twelve, and our patient was apparently dying. The pulse was between 160 and 170, exceedingly feeble and thready; the pupils strongly contracted, though not to pin-points; the respirations fifteen per minute, exceedingly irregular, mostly deep, jerking, and interrupted; the skin pale and dry; the consciousness completely lost, violent shaking and shouting in the ear only eliciting a few grunts; the temperature in the axilla 108½° Fahr.; the wrists pale, and no signs of pain elicited by violently moving them. On ausculting the heart I could find no murmur; the first sound was very feeble, somewhat prolonged, and the second, sharply accentuated.

It was very evident, gentlemen, that if this man were left to nature he would die, and that most speedily. As a contrast to what happened under treatment in the man now hale and hearty before you, and as portraying the natural history of this disorder, let me read you the following account of a case that occurred here a few weeks since, and was in a measure left to nature:

Charles M., æt. 40, of German birth, was admitted to the hospital March 6, 1874, suffering from acute articular rheumatism. On March 11 he was seemingly much better, the pain having left him, and the redness of the joints diminished; but the resident physician in the morning noticed that he was very drowsy. At 10 P.M.

the resident physician was summoned by the nurse, who said that the man had apparently taken an over-amount of opium, of which large medicinal doses were being given him. The man was found unconscious, lying quiet and motionless, with slow, rather deep respirations. He was aroused with difficulty by vigorous shaking, and was able to put out his tongue, but could not speak. He had a dull, dazed expression, and appeared like a man who was mildly narcotized, so that the resident, agreeing with the nurse, ordered strong coffee and watching.

At midnight the doctor in charge was again sent for. The patient had lost the power of swallowing, and could not be aroused at all; but he showed signs of uneasiness when roughly pinched or pricked with a pin. His respirations were 18; pulse 160. Eight ounces of blood were taken by cups from the back of his neck, three drops of croton oil administered, and external counter-irritation freely made. About 1 A.M. the resident's attention was attracted to his intensely hot skin, and on thermometrical examination the temperature was found to be 110½° F. It did not fall below this up to death, which took place about 4 A.M. A careful autopsy was made, but no lesion whatever could be found, unless it were a doubtful congestion of the brain.

In order to treat my case intelligently, it was of course necessary to determine first the nature of the frightfully sudden exacerbation which threatened his life. In the course of inflammatory rheumatism there happens every now and then exactly what occurred in our patient,—an apparent disappearance of the external symptoms, a sudden rise of temperature, the development of the most severe nervous disturbance, all ending in death. This is the so-called "cerebral rheumatism." What is its essence or nature? What the sequence and relation of its phenomena? Of the real essence or nature of rheumatism we know scarcely more than did our asserted simian progenitors before Adam was. We can, however, I think, say, in accordance with our clinical knowledge, that there is in the system a something, a poison, an irritation,—in our ignorance one term is as good as another,—an influence which flits from part to part, seemingly subject to no laws and perfectly whimsical in its movements.

Is cerebral rheumatism, then, simply the result of a retrocession of this agent to the nerve-centres? are the unconsciousness, the motor, respiratory, and circulatory disorders, simply due to a direct action of the rheumatic influence upon the nerve-centres? Gentlemen, I say no; and I hope to convince you of the truth of this denial. If the spinal cord be cut just below the origin of the respiratory nerves, and the animal, by being wrapped in cotton or by being placed in a warm atmosphere, be protected from the great loss of heat by radiation which follows the paralysis of the peripheral vessels, a rise of temperature occurs, and progressively continues until the animal dies in a frightful fever. What does this mean? To my mind, it means that there is an inhibitory heat-centre at the base of the brain, or, in other words, a ganglion, which controls and regulates those chemical changes whose outcome is "animal heat." The circulation is greatly lessened in its activity by the section of the cord, and it seems hardly probable that the stagnant blood-current of

vaso-motor paralysis should of itself cause a rise of temperature. I have, however, not time this morning to reason out this matter before you. So far as concerns the present case, the only lesson of importance is distinctly taught by the experiment,—namely, that there is somewhere in the brain a centre which controls the heat-production of the body directly or indirectly, and whose paralysis is followed by violent fever.

With this light it is easy to explain the high temperature which was present in our case. The rheumatic poison or influence did so affect this centre that upon the second day of the relapse the temperature rose to 104° F., and upon the third day, the paresis of this centre deepening into paralysis, the axillary temperature went to 108½° F.

Unfortunately, we have no observation to show whether there was a palpable transfer of the rheumatic irritation from the joints to this centre. There may or may not have been.

Having found the method in which the temperature was in this case elevated, we have, gentlemen, discovered how the nervous symptoms were produced, for I insist that they were not the result of rheumatic influence acting upon the brain or its membranes, but were solely due to the excessive heat. Do not mistake me. I do not claim that there never is a "cerebral rheumatism" in which the cerebrum is directly affected by the rheumatic agent, but I maintain that in the present case this did not happen; that the excess of heat was the poison, and that the reason the rheumatic influence was so completely lost in the joints was not so much that it had migrated from them as that it was overwhelmed and imperceptible amidst the general wreck. I may further add that, in my opinion, very many of the cases of cerebral rheumatism have the same pathology as the present. The proof which I shall adduce in favor of the view enunciated may be summed up in the following propositions: First, that the temperature present would of very necessity cause nervous symptoms exactly like those which were actually present in our patient; second, that removal of the excessive heat was followed by instantaneous relief of the nervous symptoms.

First. As is well known, exposure of man to heat produces a disease known as sunstroke; and I call your attention to the similarity of the symptoms of that disease with those of the case before us. The suddenness and the terrific force of the onset, the biting, dry skin, the high temperature, the contracted pupil, the unconsciousness, the irregular breathing, and the exceedingly rapid pulse, are all common features, and are almost the sole features of each disease: in the one case you see a counterpart of the other. I have elsewhere proven that the poison of sunstroke is heat. Time fails, and I cannot go over the experiments in detail, but can only sketch a few prominent features of my investigation. It was found that heating produces in animals a sunstroke exactly simulating the disease as seen in man. As in experimenting it was possible to eliminate all other factors, it was distinctly proven that heat is the sole cause of thermic fever. Further, by a simple mechanical contrivance the brains of animals were

heated without the remainder of the body being affected, and the same sudden loss of consciousness, contraction of pupils, and respiratory disturbance as are present in sunstroke were developed just at the time when the brain reached the point respectively attained in thermic fever in the various species of animals employed. It was found that a temperature of 113°–114° F. was fatal to the brain of a cat. The normal temperature of a cat is 102.5° F.; of a man, 98.5° F. The brain of the cat is probably less sensitive than that of man, because less highly organized. In our patient we found, late in the case, that the rectal temperature was two degrees higher than the axillary, and without doubt his brain-temperature when first seen was at least 110° F.,—11.5° F. hotter than normal. In the cat a cerebral heat-increment of 6° F. will cause violent convulsions, with unconsciousness, and one of 11° F., complete brain-paralysis.

Evidently, our first proposition is proven. By reasoning similar to that which has been gone through before you, I came to the conclusion that our patient was dying of heat, and determined to cool him at all hazards, and, as the surest and most rapid means, to employ the cold bath.

The following is the record made at the time:

- 1.24 P.M.—Patient put in a full bath at 60° F.
 1.25½.—Shows signs of consciousness; will put out the tongue when loudly asked to do so.
 1.27.—Seems to recognize the bath is very cold, and struggles to get out.
 1.30½.—Man has a fair degree of rationality. He has been in six minutes and a half, and now ordered to be taken out at once.
One minute after the bath.—The patient was partially wiped and laid directly upon a gum blanket, and covered only with a sheet, in a room whose temperature was about 65° to 70° F. He has just received a hypodermic injection of six grains of quinine.
Three minutes.—Temperature, in axilla, 94° F.; in mouth, 105½° F.
Eight minutes.—Temperature has been steadily falling; is now 103° F. in mouth. The man has become perfectly rational, and answers to his name.
 Pain and sensibility have returned somewhat to the wrist. Ice-bladders were applied to head ten minutes after bath. The attendants state that he passed a very little urine at 11 o'clock; bladder is now entirely empty.

Twenty-four minutes.—Temperature in mouth 104½°. 2 P.M.—Pulse 140, weak. One-half fluidrachm of tincture of digitalis, with two ounces of raw whisky, were given.

2.45.—Temperature in mouth 101°, in axilla 102°, in rectum 102½°.

4.15.—Digitalis and whisky were repeated, but were immediately vomited. Pulse 140; temperature 101½°. No urine had yet been secreted into the bladder. He was cupped ten ounces of blood over the kidneys. Ordered an enema, also ʒii of acetate of potassium ordered every two hours, with two ounces of infusion of digitalis applied on cloths to abdomen. Small quantities of milk and lime-water were given at intervals.

8 P.M.—The patient says he feels very much better; recollects nothing of the past, excepting that in the morning he was very dizzy, and just afterwards became unconscious. Application of digitalis renewed. Pulse 128, temperature 102½°.

12 midnight.—Pulse 116, much stronger; temperature 99½°. Complains of feeling cold, but in other

respects is better. Has passed about a pint of urine. Since his bath he has been lying on the gum blanket, covered only by a sheet, as at first. He is now ordered to be put into a warm bed, and covered with blankets. Ice still kept applied to head. Other treatment continued, excepting the digitalis-poultice.

April 9, 9 A.M.—Is much better. First sound of heart very weak; second sound very strongly accentuated, both pulmonary and aortic. There is no murmur.

Was ordered to take, during day, 3ss citrate of potassium, in one-half pint of water, every two hours; ten drops tincture of digitalis, and two grains quinine, every three hours; one-half pint milk every four hours. Temperature $100\frac{1}{2}^{\circ}$, pulse 120.

At 12, temperature $101\frac{3}{4}^{\circ}$, pulse 124.

3 P.M.—Temperature 101° , pulse 120.

8 P.M.—Treatment all carried out, excepting the milk, which caused great nausea and vomiting; it was therefore given one ounce at a time, and about one pint, with eight ounces of beef-tea, was retained during the day. Medicine was all retained without difficulty. He has passed urine freely during the day,—about three pints in all. Temperature $102\frac{1}{2}^{\circ}$ F., pulse 122. He complains of severe pain in wrist-joints and in the shoulders. Treatment continued during night, when awake. Five-grain Dover's powder ordered; the same dose to be repeated at 12 o'clock, if awake.

April 10, 9 A.M.—The patient slept during the past night, for the first time since the 8th. Took his medicine without any nausea being experienced. Says he feels much better. Ice-cloths to head discontinued; other treatment continued. The condition of joints of wrists and shoulders is about the same as on the morning of the 8th: they are very swollen and painful. Pulse 120, temperature 101° F.

6 P.M.—Condition as in morning. Has taken one quart of milk during the day. Temperature $100\frac{3}{4}^{\circ}$ F., pulse 116.

April 11.—Temperature $100\frac{3}{4}^{\circ}$ F., pulse 92. Quinine sulph., fifteen grains daily, ordered. Digitalis and the potassium salt continued.

April 12.—Is very much improved. Pulse 96, temperature 100° . Citrate of potassium ordered every three hours; thirty drops tincture of digitalis in twenty-four hours.

April 14.—Feels very little pain in joints, has good appetite, and looks, indeed, almost well. Citrate of potassium discontinued. Iodide of potassium, ten grains, t. d.; tincture of chloride of iron, gtt. xx, t. d. Quinine continued. Temperature 99° , pulse 72. Tincture of digitalis stopped.

May 4.—From the time of previous note he steadily progressed towards entire convalescence under a tonic treatment, and has been for some days going about freely. To-day he is to be discharged, perfectly well. There is no cardiac lesion whatever.

Let me call your attention, gentlemen, to the rapidity with which, under the influence of the cold bath, the symptoms abated in our patient. He was not in more than a minute and a half before he exhibited very distinct signs of returning consciousness, and in three minutes had sense enough to attempt to get out of the tub. What could the bath do to affect the man so much but withdraw the heat, which, as you know, we have found to be a poison to the nervous system? That the heat was withdrawn, the thermometer proved. If the drowsiness had been due to simple congestion of the brain, very certainly would the bath, by driving the blood from the surface, have increased the trouble. It has been distinctly proven by various

physiologists (see paper by Dr. T. Lauder Brunton, St. Bartholomew's Hospital Reports, vol. vii.) that heat directly increases the rapidity of the heart's action, and in our patient the intensely rapid pulse became slower under the influence of the bath. In what way could the bath do this, unless it were by the withdrawal of heat from the body? Indeed, without further discussion, I claim that the above record establishes my second proposition, and that it has been proven that heat was, in this case, the agency that would have hurried our patient into the grave had it not been for the friendly bath-tub. The sudden change from death to life was, to me, intensely interesting, not only because the stake was a human life, but also as a matter of a wider scientific interest.

I have often produced in the lower animals the symptoms which were present in our patient, and, plunging the animal into a cold bath, watched the fall of bodily temperature, the rise of temperature in the water, and the disappearance, *pari passu*, of the nervous symptoms.

The experiment upon man and the experiment upon the lower animal, exactly similar in their course and results, furnish a new demonstration of the falsity of that narrow and ignorant though popular philosophy which looks upon man as something physically apart from the rest of the living creation, and fails to see that, bone of its bone and flesh of its flesh, the human is subject to the same influences and dominated by the same laws as the animal.

There is one point in the treatment to which I want to call your attention, namely, that the patient was taken out of the bath before his temperature had nearly reached the normal point. I did this because I have noticed in animals that the temperature often continues to fall after removal from the bath, and, if the latter be continued too long, may go down far below the normal point. Thus, I have exposed a pigeon, having a normal temperature of $109\frac{1}{2}^{\circ}$, in a hot-air chamber, and, when the temperature had risen to $117\frac{1}{2}^{\circ}$ F., plunged the bird into water; taken it out when the temperature was $109\frac{1}{2}^{\circ}$, and in the course of twenty-five minutes seen the temperature fall to 96° , although the bird was put in a warm place. No doubt some of this excessive fall was due to the wet feathers; but I have seen the same thing in quadrupeds, and believe that in the use of the bath in cases like the one now under consideration there is a possibility of carrying the effect too far, and of actually depressing, to a fatal extent, heat-production. In ordinary fevers this danger is, of course, less than when the original rise of temperature has been from a transient cause.

In most cases of cerebral rheumatism there is a very manifest tendency for the reduced temperature to rise again, and the question is a most vital one why, in the present instance, no such inclination showed itself. Very probably the hypodermic injection of quinine had some decided influence, since, although the alkaloid has very little effect in reducing normal temperature, it does appear to exert a very decided antipyretic action when the temperature is above normal. I believe, however,

that the prevention of relapse was largely due to what seems to me the boldest part of the treatment. Remember, the man was suffering from acute inflammatory rheumatism, and, a few hours before, a council of our most experienced residents had diagnosed commencing pericarditis; yet the patient was plunged into a cold bath, taken out of it, and, without being wiped dry, laid upon a cold rubber blanket, covered only with a sheet, and so left in a cool room for many hours. What seemingly could be better calculated to bring on fatal internal rheumatic inflammations? Yet no pericarditis developed itself; only a steady convalescence, from a condition formerly considered almost of necessity fatal.

What was the reason? I believe it was partly because the circulation was steadied by large doses of digitalis, but chiefly on account of the high temperature. How can you chill a man whose body is burning up, and whose circulation is filling every extreme arteriole and capillary under the greatly increased arterial pressure caused by the digitalis? I do not want, however, to see you recklessly follow the treatment of the present case, at least so far as concerns the after-exposure of the patient.

"One swallow does not make a summer," and in departing so widely from the old paths we are treading untried ground. If I had been on the spot, this exposure would not have been so long continued. When the temperature showed indications of falling below 102°, the man ought to have been placed in bed and lightly covered.

The case, gentlemen, would afford much food for further discussion. It would be especially interesting to study the use of digitalis as a stimulant, in this connection; but our hour has expired, and the record must be left to tell its own story.

ORIGINAL COMMUNICATIONS.

THE LOCAL TREATMENT OF PULMONARY CAVITIES BY INJECTIONS THROUGH THE CHEST-WALLS.

BY JAMES H. HUTCHINSON, M.D.,

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THE attention of the readers of the *Philadelphia Medical Times* has been recently called to this method of treatment of the third stage of phthisis pulmonalis, by Dr. William Pepper, who resorted to it in three cases with great advantage, he thinks, to the patients. By a coincidence which is not infrequent in medicine, Dr. Mosler has been trying the same experiment in Germany, and has published an account of it, which reached this city after Dr. Pepper's paper was in press. It is not necessary to give details of these experiments further than to say that Dr. Pepper injects into the cavity, by means of Dieulafoy's aspirator, from four to ten minims of a very dilute Lugol's solution of iodine (M_{iv} to ℥i), and that Dr. Mosler, after making a perforation, —the means by which this was done are not given, —introduced a silver drainage-tube, which appears to have been allowed to remain in the body until the death of the patient, four months after the

operation. During life, the tube was not merely used to permit the escape of secretions, but also served as an avenue to introduce various medicinal substances into the cavity. We of course recognize a difference between these two sets of experiments, inasmuch as in one the employment of the aspirator probably prevented the entrance of air into the cavity, and in the other this was allowed to take place freely. We cannot, however, regard this as a very important distinction, since there is almost always an uninterrupted communication through the bronchi between excavations in the lungs and the outer air. Moreover, Dr. Pepper says, in concluding his paper, that he should have no objection to use a trocar and canula, and to leave the latter permanently in the wound.

We need not say how cordially we should welcome any operation or remedy which afforded a reasonable hope that it would be found to be useful in reducing the mortality from phthisis, or even in prolonging life after the formation of vomicae. For, although we do not admit that the existence of a cavity is necessarily an indication of a speedy dissolution, we agree with Dr. Pepper in thinking that when the disease has arrived at this stage we possess but little power to stay its downward course. Both of our experimenters speak of their plan of treatment as affording this hope, and, moreover, appear to think that it is novel. It is in these points of view that we propose to criticise it; and first, in regard to its novelty.

In the London translation of Baglivi's *Practice of Physick*, published in 1723, we find the following passage, which we quote just as it is printed:

"A Phthisick arising from an Ulcer is commonly branded as incurable, upon the Plea that the Ulcer is internal and occult, and cannot be cleansed like other external Ulcers. But why do they not make it their Business to find out the true Situation of the Ulcer, and make an Incision accordingly, between the Ribs, to the end that proper Remedies may be conveyed to it? For my Part, I know no Reason why that should lie neglected. About Seven Years ago, when I was at Padua, a Man received a Wound in the Right side of his Breast, that reached to his Lungs; and, employing an able Surgeon, had an incision made between the Ribs to the Length of Six Fingers' breadth, in order to discover the Situation of the Wound in the Lungs, which was perfectly cured in Two Months' Time, with Vulneraries, applied with Tents and with Syringing. Now, Practitioners ought to use the same Piece of Diligence in curing a Phthisical Ulcer in the Lungs, lest the Scroll of incurable Diseases should grow too long, to the infinite Disgrace of the Profession."

As Baglivi's work was published in 1696, we have sufficient evidence in the above that the propriety of establishing a communication between the lungs and the outer air through the walls of the chest, for the purpose of applying remedies directly to the seat of the disease in cases of phthisis, was entertained as early as the seventeenth century. We shall show in the following quotation from a work published in London in 1763, by Dr. Edward Barry, F.R.S., entitled "A Treatise on the Three Different Digestions," that the operation was recommended by Hippocrates, and that it was not infrequently done during the eighteenth century:

"When evident marks," Dr. Barry says, "appear in the Sides, all Authors, antient and modern, agree, that an Aperture should be made; and Books of Observation abound with Instances of this kind, where it has been performed with Success. Hippocrates* constantly directs it either with the *Knife* or actual *Cautery*, not only where the *external Appearances* are evident, but where they are very *obscure*: and seldom seems to doubt of Success, but in very advanced Cases, where the Fluids in general and the purulent Matter collected are degenerated into a putrid eroding State, and where, though a free Discharge is given, yet the Ulcer can never heal. It appears from the Dissection of those who have died of this Distemper, that such Abscesses or Ulcers in the Lungs generally adhere by a large Surface to the Pleura. . . . By this Adhesion of the Lungs to the Pleura, the Parts affected are kept (as if carefully designed by Nature) in a great Measure in a State of Rest and easily admit an Opening into the Cavity; neither if the Operation fails can it be attended with any great Danger, or much accelerate the Progress of the Distemper."

Passing to the present century, we find that substantially the same method of treatment has not only been proposed, but actually carried into effect, many times during the last fifty years. Ramadge reports several cases in his treatise entitled "*Consumption Curable*" (London, 1836). In one of these he introduced a trocar between the second and third ribs, in a line nearly perpendicular to the left nipple. "I kept," he writes, "the punctured place open for about ten days by the introduction of a small piece of catgut properly secured externally, when, finding that the cavity became so contracted through the encroachment made on it by the pulmonary expansion as to preclude all further escape of air, I withdrew it." Dr. Hastings and Mr. Stork† have also reported one case in which a silver tube was first introduced, and, not answering, an elastic gum catheter was substituted for it; and a Mr. Robinson another. The publication of the former led to some little controversy as to the propriety of the operation, Drs. Herbert‡ and Campbell§ maintaining that no good results could be expected from it. We have also abundant proof that the accidental opening of a phthisical cavity through the chest-walls may not only not cause immediate death, but may occasionally lead to an apparent arrest of the disease. Thus, as a consequence of the application of the actual cautery to the chest in a case reported by M. Bricheteau,§ a pulmonary cavity was opened and emptied of its contents. Fifteen months later the patient succumbed to an attack of pericarditis, when the cavity was found partially obliterated. In Canstatt's Jahresbericht for 1843, vol. iii. p. 365, will be found a reference to the *Medical Times*, Nos. 60, 171, and 180, in which a writer under the signature of "Discipulus" reports two cases of phthisis, in one of which a cure is said to have been the result of a self-inflicted wound of the neck by a would-be suicide, and in the other followed the spontaneous évacuation of a vomica through the parietes of the chest.

These few extracts and references—and they

might be multiplied without difficulty—prove that the operation of injecting pulmonary cavities through the chest cannot be in any sense considered a novel one, and we believe we shall be able to show that it is not one which is likely to result in good to the patient. In the first place, the presumption in regard to its usefulness is against an operation which, after having enjoyed a brief popularity during at least three or four different periods, has been so utterly forgotten that it has been as many times proposed as entirely new, and is certainly against one in which the opportunities for performing it would be so frequent as they are in this. The quotations|| which we have introduced above demonstrate with sufficient clearness that the lung may in many instances be laid bare and punctured without apparent injury to the patient. We are inclined to think that this is by no means uniformly the case. In one of the cases reported by Dr. Pepper a slight hemorrhage followed the introduction of the aspirator; and the same accident occurred in the operations done by Dr. Mosler and Dr. Hastings. We are, moreover, very strongly convinced of the fallacy of the argument that, because punctured wounds of the lungs in healthy individuals generally do well, their infliction upon consumptives is not likely to be followed by bad results. Niemeyer—we believe (for we quote him at second hand)—goes so far as to say that the inflammation excited by wounds of the lungs usually terminates in phthisis; but, admitting that this distinguished physician may push his conclusions as to the nature and origin of this disease to an unwarrantable length, there are certain differences in the anatomy of the lung in health and disease which ought not to be overlooked. Caseous degeneration is not the only change which is observed in phthisis. There is in addition to this, in most cases, a development of connective tissue, the effect of which will be to enlarge and keep open the wound made by the aspirator or trocar, and thus to allow the escape of the contents of the cavity into the pleural sac whenever this is not prevented by firm adhesion; and that cavities are not always protected by adhesions is shown by the occasional occurrence of pneumothorax from their rupture. Moreover, the opinion that injections may be of service in the treatment of phthisis rests, we think, upon a mistaken therapeutic basis. In certain conditions of the serous membranes these are unquestionably useful by exciting inflammation; but a little reflection will convince any one who has abandoned in whole or in part the theory which makes phthisis the result of tubercular deposit that this is the very last thing to be desired in this disease. The object aimed at in the management of phthisis, and especially in those local cases in which Dr. Pepper thinks the injections are most likely to be useful, is to allay inflammatory action, not to excite it. We therefore are unable to see how they can be productive of any good; on the contrary, if they light up an inflammatory process in the walls of the cavity, this will be very likely to extend to the cir-

* De Affect. S. V.

† London Medical Gazette, 1845.

‡ Lancet, 1845.

§ Valleix, Guide du Médecin Praticien.

|| See also Van Swieten's Commentaries, English translation, 1759, Section 170.

cumjacent tissue, and thus the disease, which may have been previously held in abeyance, be roused into activity.

In conclusion, we have only to add that we never condemn a plan of treatment on purely theoretical grounds, and will gladly adopt the one under consideration if the result of further experiments shall convince us of the incorrectness of the unfavorable opinion we have formed of it. In the mean time, however, we shall remain in full accord with Dr. Hughes Bennett,* who thinks that the result of all operative interference in phthisis has been "what an intelligent consideration of the pathology of the disease might have anticipated,—a uniform failure."

2019 WALNUT STREET.

AN UNNATURAL POSITION OF THE HEAD A CAUSE OF DEATH FROM CHLORO- FORM AND OTHER ANÆSTHETICS.

BY G. W. COPELAND, M.D.,

Boston.

IN the *Boston Medical and Surgical Journal* of February 26, I published a short article on the "Styloid Muscles and Anæsthetics," in which I referred the cause of impeded breathing during anæsthesia to the action of the styloid muscles closing the glottis. I also pointed out how the difficulty could always be relieved without making traction on the tongue, simply by tilting the head forward in a natural position, so as to relax these muscles and permit the patient to breathe through the nose.

It has since occurred to me that many of the deaths which have resulted from the use of anæsthetics may have been due to the imperfect knowledge that has existed regarding this matter. The difficulty experienced in keeping the air-passages free has, ever since the discovery of anæsthetics, been the most troublesome and dangerous complication attending their use. All authorities agree that obstructed or impeded respiration is a symptom attended with great danger to the life of the patient, and always requires prompt and immediate attention.

Waring, in his book on Practical Therapeutics, sums up the experience of the profession with chloroform, in the following observations: "Watch carefully the respiratory movements, and the color of the cheeks, lips, and eyes. Signs of danger: lividity of the face, stertorous respirations, irregular, gasping respirations, feeble pulse, death-like pallor." His directions are, "Stop the chloroform, open the mouth, draw out the tongue, and watch carefully."

When we consider the ways in which deaths occur during anæsthesia, we see that these signs of danger have not been so designated without sufficient reasons.

Gant, in his *Science and Practice of Surgery*, speaks of death from chloroform as follows: "During inhalation, death may occur in three different ways: by asphyxia, by cardiac syncope, or by coma.

"Asphyxia is indicated by the ordinary symp-

toms, lividity and turgescence of the face, violent respiratory efforts, and cessation of the pulse and of the heart's action."

In cardiac syncope "the patient, after a few inspirations, suddenly becomes pale and faint, the pulse beating almost imperceptibly for a few moments, and then ceasing, although the respirations may continue; death taking place by paralysis of the heart."

"Coma presents the same appearance as asphyxia, but without failure of the heart's action; death resulting from congestion of the brain."

In two of these three modes of death we then see that there is impeded breathing and improper aeration of the blood, as evinced by the lividity of the face. The great mistake up to the present time has been in considering the difficulty with the breathing, a symptom arising from the use of anæsthetics or a sign of full anæsthesia, and not a mechanical obstruction of the glottis from an unnatural position of the head.

The *Lancet* of February 1, 1873, contains a report by the editor of a "Case of Fatal Suffocation from Nitrous Oxide Gas," in which a dentist is related as having administered that agent for the purpose of extracting a tooth. He says "it was not till after the operation was completed that anything unusual happened, but immediately afterwards the face became livid, and the 'features,'" it is said, "commenced to 'swell.'" He refers death in this case to asphyxia.

In a case of death from the bichloride of methylene, which occurred at the Charing Cross Hospital in the practice of Mr. Canton, it is stated that "one nostril was closed by the tumor, thus obstructing respiration, and the patient was subjected to the influence of the bichloride in the sitting posture."

In the *Lancet* for September 6, 1873, the editor, in commenting on a death from chloroform which happened in a dentist's chair at Brighton, the post-mortem showing fatty degeneration of the heart, says, "It is one of those cases when death seems to have followed rigidity of muscle, and it is questionable whether asphyxia may not have helped, as it were, the fatal syncope."

It seems to me very plausible that cardiac syncope may be induced by the momentary closure of the larynx depriving the heart of its natural stimulus in patients suffering from shock or fatty degeneration, or already reduced by disease. It is a significant fact that all the deaths from nitrous oxide gas, and a large number of those from other anæsthetics, have taken place while the patients were in a sitting posture, which would allow the head to fall back farther than if they were lying down, thus favoring the theory that interference with the free action of the lungs may have been the primary cause of death. It must be remembered that closure of the glottis may occur without our attention being directed to those struggling efforts on the part of the patient which would happen were voluntary efforts possible, and the first noticeable symptoms of danger may be lividity of face or death-like pallor.

It may not be out of place here to refer to the many deaths from intoxication, apoplexy, and

* Reynolds's System of Medicine, vol. iii. p. 589.

coma from whatever cause, as being in some instances hastened, and perhaps even produced, by interference with the respiratory functions from an unnatural position of the head.

There is one other point to which I wish to allude, and it is the importance of elevating the head sufficiently to compel the patient to inhale the anæsthetic through the nares entirely. If deep inspirations be taken through the open mouth, the lungs are inflated instantaneously and just as rapidly emptied, leaving a long interval without any of the vapor being in the lungs. If the inhalations be through the nares, it takes a much longer time to inflate the lungs and a much longer time to empty them, leaving no interval. Now, the number of respirations per minute is the same either way: it follows that it will require a longer time to effect anæsthesia through the mouth than through the nose.

It appears evident to me that if attention be paid to the position of the head during anæsthesia from any agent, a greater degree of safety will be obtained than has hitherto existed.

NOTES OF SOME SURGICAL CASES.

BY JOHN J. BLACK, M.D.,

New Castle, Delaware.

FEMORAL HERNIA, AT TIMES SIMULATING STRANGULATION, ACCOMPANIED BY A CLINICAL CURIOSITY.

SATURDAY, November 29, 1873, 11 o'clock A.M., I was summoned to see a lady about 60 years of age; she told me she had had a "rupture" in each groin for a number of years. Never had had trouble until this morning, when she was unable to reduce the one on the left side. Had severe umbilical pain, and the tumor was tender. Had worn a double truss "off and on" for a long time. Upon examination, I found a femoral hernia on each side; the right one normal, the left one presenting a tumor, size of a hen's egg, quite tender, and irreducible after the usual manipulation. I then asked my friend Dr. G. T. Maxwell to see the case with me. We again tried manipulation without success. Then we etherized the patient, and were still unsuccessful. We introduced the needle of a hypodermic syringe, and drew off a little serous fluid, but do not think the bulk of the tumor diminished much. We next gave a full opiate, and our patient was comfortable for several hours. Did not vomit. Again the tumor did not yield to manipulation, very carefully performed, yet the symptoms of strangulation were not decided, and we gave her an opiate, and left her until morning. Then I found her with pain about the umbilicus, and vomiting; and then I made up my mind there was strangulation, and I would have to cut down on the tumor and liberate it. But, after giving her numerous draughts of water, the stomach quieted, pain ceased, and the symptoms were no longer urgent, the sickness and bilious vomiting having been caused by the opiate. The tenderness increased in the tumor; redness appeared, showing a tendency to suppurate. This state of affairs continued increasing until December 10 following, when I made carefully a valvular incision and liberated a large amount of pus. Immediately on opening the tumor there was an escape of the foulest-smelling gas. I supposed at the time I had unintentionally opened the bowel; but later disclosures have taught me to believe that the foreign body hereafter to be spoken

of had penetrated the bowel from within outwardly, and caused the suppuration in the cellular tissue of the abdominal walls, external entirely to the bowel. Poultices were applied for several days, rest enjoined, liquid diet, etc.

One morning, about ten days after this, on visiting her, she showed me a piece of bone, about one and a half inches long, which she had extracted from the opening in the tumor. After this the opening soon healed, and up to this time (April 17, 1874) the hernia has appeared to be "radically cured."

Remarks.—My friend Dr. James Tyson, of Philadelphia, kindly presented the bone to Prof. Harrison Allen for examination, and he pronounced it to be not human, but a part of the rib of a robust-chested mammal of small size. Prof. Leidy first thought it a foetal rib, but, after examining it, was satisfied it was not human. I may add here that the patient was a married woman, and had had two children.

This case is interesting, first, as simulating strangulation, which I believe did not occur at any time during its progress; second, the discharge of the foreign body, which must have been swallowed with the food, and have worked its way through the bowel; and third, as exhibiting the hernia as up to date cured by the inflammation developed in the case.

AMPUTATION AT THE SHOULDER-JOINT FOR EXTENSIVE GUNSHOT WOUND OF THE ARM AND SHOULDER.

Friday, October 31, 1873, I was called down the State by Dr. Kemp to see, in company with Drs. Dunlop, Worrell, and Fromberger, a young gentleman, 23 years of age, healthy, and well built, who, the evening before, had, whilst returning from a duck-shooting excursion, shot himself accidentally with a full load of heavy duck-shot through the upper part of the left arm and shoulder. The bone was shattered nearly to the head; soft parts carried away on top of arm and shoulder, and also part of the skin and muscles of the shoulder posteriorly, leaving the arm hanging by the axilla, vessels, and nerves. He was very weak, and had apparently lost much blood before assistance had been rendered him, but, thanks to the attention of the physicians in charge, had commenced to rally, and was not losing blood. At the time of my arrival, of course, there was nothing to do but to take out the arm at the shoulder-joint, which, with the approbation and assistance of the medical gentlemen present, I did, after the method of Larrey, modified somewhat, inasmuch as the soft parts were so much lacerated and destroyed. I was compelled to draw the parts together posteriorly, at right angles to the usual single-line wound left by the operation. The patient passed through the ordeal moderately well. The pulse afterwards was very weak, and he hung between life and death for several days, but finally made a good recovery, thanks to the skilful management of the after-treatment by Dr. Kemp and the gentlemen mentioned, and the kind care of his friends.

Remarks.—This case, at first sight, was certainly a very unpromising one, and I publish it to add it to the statistics of shoulder-joint amputations, and as illustrating what to me has always appeared a fact,—that shoulder-joint amputations are among the most successful, in their final termination, in surgery; and now, when I have occasion to watch the progress of such cases, I always feel highly assured that "le bon temps viendra."

SILK LIGATURES REMAINING UNUSUALLY LONG AFTER AMPUTATIONS.

Some time since, I had occasion to amputate a forearm for gunshot wound. I applied four ligatures. Those on the radial and ulnar arteries remained six months. After some time they became deteriorated and broke off on pulling them. Little abscesses formed, and in about six weeks the knots came away, and left a good stump.

Again, I amputated a boy's leg at the knee-joint, and the main ligature acted in the same way, but came away at the end of three months, and the stump was an admirable one. I suppose the ligatures must have embraced some tendinous substance, together with the artery; and such cases should teach one to be, as I thought I was, careful in applying ligatures.

TRACHEOTOMY FOR SUPPOSED FOREIGN BODY IN THE TRACHEA.

Some time since, a woman came hurriedly into my office with a little girl, about 18 months old, in the last gasps of suffocation, telling me the child had swallowed a groundnut kernel the "wrong way." I immediately opened the trachea in the usual manner, the attempts at respiration being so feeble as to impede me but little. I could find nothing above or below, as far as I could go. All my attempts at artificial respiration, etc., proved ineffectual, the child having died by the time I got the trachea opened. I suppose the nut-kernel slipped into one of the bronchi as the parts became relaxed by death, having been tight enough before to produce suffocation. I could not obtain a post-mortem.

CAUTION AGAINST THE USE OF ETHER NEAR FIRE OR LIGHT.

Some time since, in removing a tumor from a woman's neck, I had occasion to use the actual cautery, and whilst doing so the ether on the towel took fire, and disconcerted for a moment all present, but did no damage except very slightly burning the patient's face. Fortunately, the bottle did not come in contact with the flame. I remember almost serious trouble to have arisen from this accident in this very way in the clinic of a distinguished surgeon. I mention it in order that it may serve its part in this important matter, and keep all on their guard when using ether near fire or light.

[The light should always be *above* the ether, the vapor of which is heavier than air and always falls.—ED. *P. M. T.*]

SACCHARATED CALOMEL.

BY C. G. POLK, M.D.,
Philadelphia.

IN this age, so fruitful in the invention of means for "killing bodies and saving souls," as Byron tells us, it is useless to be surprised at any innovation that may be brought forward; some of these, however, are supremely ridiculous, and among these is saccharated calomel,—the last new sensation. The uninitiated would infer from our journals that its discovery will materially enlarge the domain of medicine, and prove a panacea for all the ills to which flesh is heir.

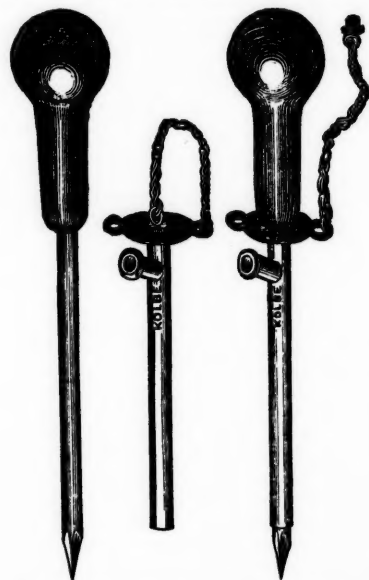
About eight years ago, while on duty with the Seventh U. S. Infantry, I had some calomel and sugar rubbed together to facilitate its administration. About a month afterwards my hospital steward administered ten grains of this calomel to a soldier. Burning and intense pain in the stomach, with considerable nausea, followed. I administered albumen,

with an emetic, and relieved the poor fellow. On testing the saccharated calomel, I found it contained both calomel and corrosive sublimate. If any one wishes to test this, let him mix the sugar and calomel, let it stand a month or more, mix with water, pour off the water, and add a little milk of lime to it, and the yellow tinge will become obvious, showing the presence of the bichloride. In the case referred to I tested the unmixed calomel, and found it entirely free from the presence of corrosive sublimate. The *Chemical News*, November 1, 1872, says, "G. Vulpius has been examining this subject, and finds that when calomel is mixed in powder with white sugar, or calcined magnesia, or hydrated carbonate of magnesia, or bicarbonate of soda, corrosive sublimate is formed in twenty-four hours. Rather large quantities are formed in powders composed of calomel, white sugar, and bicarbonate of soda."

AN IMPROVED TROCAR.

BY THOMAS G. MORTON, M.D.,
Surgeon to the Pennsylvania Hospital.

IN performing ordinary tapping operations, it frequently occurred to me that the silver canula of the common trocar might be improved by having at its upper extremity a short additional silver tube, upon which rubber tubing of any length could be attached. Mr. Kolbe made for me, nearly a year ago, the instrument shown in the adjoining cut, which I have, with others, used with great



satisfaction. After the trocar has been withdrawn, the metal plug, which is attached to the chain, is introduced into and accurately closes the end of the canula; the fluid is thus directed through the shorter tube, and conveyed away by rubber tubing.

In hydroceles, ovarian and abdominal dropsies, this addition to the common trocar has been found very satisfactory, especially in those cases where the

patient cannot be placed in the usual position for tapping, while the bed and surroundings of the patient, so liable to be soiled in using the ordinary trocar, can be with this instrument entirely protected during the removal of the fluid.

NOTES OF HOSPITAL PRACTICE.

BELLEVUE HOSPITAL, NEW YORK.

Reported by F. W. CHAPIN, M.D.

A CASE OF STRANGULATED HERNIA.

A. S.; 47; male; U.S.; carman; admitted August 18, 1873. Patient has always been healthy, and moderately temperate. Four years ago he became aware of the existence of a hernia on the left side. The tumor was of about the size of a walnut, but has since increased to the size of an orange.

About 6 P.M. yesterday he broke his truss, but continued to work, lifting heavy iron rods. The hernia came down, and increased very rapidly in size. He attempted to reduce it himself several times. During the night he occasionally experienced slight pains in the abdomen, but did not vomit.

This morning a physician tried in vain for half an hour to reduce the hernia.

On admission, 1.30 P.M., he states that he has eaten almost nothing since last night; that since morning he has had some nausea, with increased abdominal pain; that he has not vomited, and that his bowels have not been moved for two days. His general condition on admission is good; pulse 80, regular and full. Tumor measures 13½ inches from the peno-scrotal junction to the internal ring, and 12½ inches transversely at its broadest part. He was put in bed; foot of bed elevated; ice placed over the tumor. This treatment was continued till 3.30 P.M., when the tumor was aspirated, once at the lower part, and again in the middle. No fluid was obtained. At 3.40, and again at 5 P.M., taxis was tried unsuccessfully. The patient was then etherized, and an incision made over the middle of the tumor from the point opposite the internal abdominal ring to the peno-scrotal junction. The layers were carefully divided on a director until a gush of a considerable quantity of sanguinolent serum indicated the opening of the sac, which was then slit up. A layer of omentum was exposed, of dark color, thickened, and exhibiting on its surface numerous turgid veins. This being laid back, a loop of intestine, nine or ten inches in length, was seen; normal in color, odor, and feel. Just beneath the intestine was a recent clot of blood, of about the size of a small hen's-egg. The constriction was found at the external ring, and divided. The internal ring was large, and had been dragged down so that the two were nearly opposite each other. Adhesions were discovered between the under surface of the intestine and the hernial sac, preventing the reduction of the former. These were divided, partly with the knife, and partly by tearing. Several small vessels which bled quite freely were closed by torsion, and one was tied with silver wire, the ends being cut short, and the remainder subsequently returned into the cavity of the abdomen with the gut. The omentum was tied with five silk ligatures, and the hernial portion then cut off. This portion was as large as an ordinary man's hand. The ligatures being firmly held, the omental stump was allowed to pass back into the abdominal cavity, and after further manipulation the intestine was also returned. The ligatures were brought out at the upper

end of the wound, and the latter, after excision of a portion of the sac, was closed with wire sutures. A compress of lint was then firmly bandaged over the inguinal canal. The operation had lasted about an hour. Pulse at the close was 96, regular, and somewhat compressible. Respiration 20; temperature 99°.

9 P.M.—Patient rallied well from the ether; is now quiet and comfortable. Has very little pain in the region of the wound; none elsewhere. Was given hypodermic of Magendie $\mathbb{M}x$ at 7 P.M., and another at 8.30 P.M.; pulse 88, respiration 16, temperature 100°.

August 11.—Pulse 88, respiration 16, temperature 100°. Patient had $\mathbb{M}x$ of Magendie again at 12 M. last night, after which he slept well. This morning he says he feels well; has slight pain across the abdomen; no tenderness. Tongue slightly coated. Pupils contracted. He has not vomited since the operation.

10 P.M.—Patient has vomited two or three times after taking food. He has been kept under the influence of opium all the time. Pulse 120, respiration 16, temperature 102°. Slight pain and tenderness in abdomen, with marked tympanites.

August 12, 9 A.M.—Pulse 98, respiration 18, temperature 100°. Patient vomits a good deal; the vomited matter is bitter. Ordered

R Acid. hydrocyan. dil.,
Spts. chloroformi, aa $\mathbb{M}xxvj$;
Aq. calcis, $\mathfrak{z}ij$.—M.

Sig.— $\mathfrak{z}i$ q. 2 h.

5 P.M.—Pulse 84, respiration 26, temperature 100½°. Some tympanites and tenderness. No vomiting since morning. Patient continues to take morphine quite freely. Nourishment is given frequently in liquid form.

August 13, 8 A.M.—Pulse 84, respiration 16, temperature 100½°. Patient feels well; no pain; slight tenderness and tympanites of abdomen; no vomiting. Takes nourishment readily,—beef-tea and egg-nog, $\mathfrak{z}ij$ q. 2 h., alternating. Dressings removed from wound. Scrotum but little swollen. Union exists in wound except at upper part, where the ligatures protrude, and about an inch at the lowest part. There is slight suppuration at each of these points. Pad over inguinal canal reapplied; scrotum supported by a band of adhesive plaster passing under it from one thigh to the other. Treatment by morphine and liquids continued.

7.30 P.M.—Pulse 92, respiration 18, temperature 102°. Patient has delusions; answers questions rationally.

August 14, 9 A.M.—Pulse 86, respiration 20, temperature 98½°. Delusions continue; patient feels well; no pain; no vomiting; some abdominal tympanites; some tenderness at lower part of abdomen, most marked on the left side on deep pressure. Scrotum slightly swollen and œdematous. Stitches removed from wound; union throughout, except where the ligatures protrude. No pus collected in scrotum. Morphine and liquid food continued.

4 P.M.—Pulse 86, respiration 20, temperature 99½°. Delusions and hallucinations continue; patient gets out of bed unless watched closely.

August 15, 9 A.M.—Pulse 88, respiration 22, temperature 98½°. 5 P.M.—Pulse 96, respiration 28, temperature 99½°.

Patient is delirious; is sweating profusely; has marked muscular tremor. Friends state that he had been drinking hard for a long time previous to admission.

August 16, 9 A.M.—Pulse 112, respiration 30, temperature 100½°. Patient still delirious; constantly talking and struggling. Tongue dry and coated brown; teeth covered with sordes. He complains of thirst. Wound looks well; swelling of scrotum diminished. Patient had a copious discharge from the bowels this morning. No abdominal pain. Very little tympanites. No ten-

derness except near the wound. Ordered nourishment to be continued as usual, with $\frac{3}{4}$ ss of brandy q. 2 h.

5 P.M.—Pulse 100, respiration 24, temperature $100\frac{1}{2}^{\circ}$. Patient had another passage from the bowels; is still delirious. Ordered pot. brom., gr. lx; cum chloral hydrat., gr. xx; to be repeated in four hours if sleep should not ensue.

August 17, 9 A.M.—Pulse 100, respiration 24, temperature $99\frac{1}{2}^{\circ}$. Bromide and chloral were given at 7 and 11 P.M., yesterday, and patient slept well all night. P.M.—Pulse 88, respiration 26, temperature $99\frac{1}{2}^{\circ}$.

Patient has been quiet, though slightly delirious, all day; has no abdominal pain or tympanites; is allowed to take solid food.

August 18, 9 A.M.—Patient is quiet and more rational; slept well all night. Tongue moist and thinly coated. Bowels moved last night. Slight suppuration at the point of exit of the ligatures; elsewhere union is firm. From this time the patient progressed satisfactorily.

The first omental ligature came away on August 29, and the last one was removed on September 10. On October 10 the patient was discharged and told to wear a truss, although there were no signs of a hernia.

This case presented some instructive features. The hernia was a large one, the sac containing both intestine and omentum,—a large piece of each. It was the omentum which suffered chiefly from the strangulation. And this was a fortunate circumstance; for, had the intestine suffered equally, the case would certainly have been a hopeless one. A large piece of omentum covered the gut, forming a sort of cushion for it at the seat of constriction, and thus preventing its complete strangulation by the firm, sharp, and unyielding boundaries of the external ring, until surgical interference gave relief. The omentum was found to be on the verge of sloughing. The intestine beneath it was "normal in color, odor, and feel."

Had the space occupied by the omentum been filled by intestine, it is plain that some portion of the latter would have suffered as the omentum did. The prognosis, as it was, however, was necessarily doubtful; for it was almost impossible to do no harm to the intestine while tearing and cutting it away from the sac, to which it was bound by firm adhesions. And again, the returning of so large a stump of omentum ligated in five sections, each ligature embracing a considerable amount of tissue, was a doubtful, though unavoidable, experiment. It was a month before the omental ligatures had all come away.

In spite of the drawbacks already mentioned, and of a severe attack of delirium tremens, the patient, *whose gut had been but little injured*, made a most satisfactory recovery.

THERAPEUTIC NOTES.

NEW MODE OF ADMINISTERING IODINE.—A combination of iodine with some organic substance of such a nature as to allow, by its decomposition in the economy, the liberation of this element, has long been a desideratum.

Such a combination M. Collas thinks he has found in iodized albumen, which combines ease of administration with certainty in composition. The presence of iodine in this compound cannot be distinguished until after the destruction of the organic matter. Iodized albumen is prepared by shaking briskly together a solution of albumen with iodine, in very fine powder, or in solution in an appropriate vehicle. The mixture, at first dark brown, becomes decolorized after some hours of contact, and no longer gives a violet color with

starch. The product is then dried at a gentle heat and made into pill form, divided in such a manner that each pill shall contain three-fourths of a grain of iodine.

DIGITALIS IN PUERPERAL CASES.—Dr. Winkel, who has made various experiments with the different preparations of digitalis, does not hold it to be a specific in puerperal fever, but administers it as a prophylactic against the phlegmonous process. Where disturbance of the digestion results, he discontinues its use. Dr. W. found the hypodermic use of digitalin dissolved in equal parts of alcohol and water, in the proportion of .001 gramme of digitalin in 3 grammes of fluid, to be the best method of administration. The mean requisite dose is .005 gramme. The advantage of this preparation is found in the smallness of the dose and the ease with which its action may be regulated. The effect follows in one to three days generally, a simultaneous sinking of temperature and pulse being observable. Elimination cannot be absolutely stated to take place through any organ. Digitalin appears to act upon the brain, producing rest, better sleep, and enlargement of the pupil. It appears also to act as a styptic, recommending itself in this respect after severe hemorrhage.

The question as to the tolerance by puerperal women of large doses cannot as yet be answered, the assertion of its only partial absorption through the altered condition of the intestine being unsupported by proof. The favorable action of digitalis is attributed by Dr. W. to its effect upon the circulation; for, as in the chills of the puerperal condition the prognosis is more favorable when the pulse is not over 100, it is evident that a remedy which will prevent such increase must be beneficial. The observation of after-pains following the continued use of digitalis, together with its styptic action, has given rise to the conjecture that its effect is upon the unstriated muscular fibres of the uterus. By its contraction of the capillaries it prevents exudation of the colorless blood-corpuscles.

PICRATE OF AMMONIA IN INTERMITTENT FEVERS.—Dr. Beaumetz, who some time previously had urged the use of this remedy before the Société de Thérapeutique, again mentioned at a recent meeting the success which he had attained by its administration in the treatment of rebellious intermittents after the entire failure of sulphate of quinine. In his hands it had failed but once, while its success in many other instances was remarkable. Two-thirds of a grain daily is said to be sufficient.

JABORANDI—A NEW SUDORIFIC AND SIALAGOGUE.—Dr. Coutinho, of Pernambuco, has introduced to the notice of the profession a plant hitherto known only in Brazil. According to the testimony of Dr. Rabuteau, this plant possesses the most decided diaphoretic qualities, and, in addition, acts as a powerful sialagogue. An infusion of fifty grains of the powdered leaf in about six ounces of water, although partaken of when cold, produced within three-quarters of an hour profuse sweating, accompanied by free salivation.

M. JACQUES DE MAY, a Parisian pharmacist, has devised an ingenious method of applying various ointments with accuracy to such semi-cavities as the conjunctiva, etc. The ointment, which should be mixed with twenty per cent. of oil to insure fluidity, is simply placed in a flexible tube of tin, such as is used by painters for moist colors. By this means it is kept from contact with the air, and may be squeezed out in a vermicular cylinder and directed accurately and in measured quantity just where it is wanted.

HYPODERMIC USE OF ERGOTIN.—M. Drasche, Rudolf Hospital, Vienna, uses the following hypodermically: ergotin, five grains, in a fluidrachm of glycerin; dose, twelve minims.

PHILADELPHIA MEDICAL TIMES.

A WEEKLY JOURNAL OF
MEDICAL AND SURGICAL SCIENCE.

The Philadelphia Medical Times is an independent journal, devoted to no ends or interests whatever but those common to all who cultivate the science of medicine. Its columns are open to all those who wish to express their views on any subject coming within its legitimate sphere.

We invite contributions, reports of cases, notes and queries, medical news, and whatever may tend to increase the value of our pages.

All communications must bear the name of the sender (whether the name is to be published or not), and should be addressed to Editor Philadelphia Medical Times, care of the Publishers.

PUBLISHED EVERY SATURDAY BY

J. B. LIPPINCOTT & CO.,

715 and 717 Market St., Philadelphia, and 25 Bond St., New York.

SATURDAY, MAY 30, 1874.

EDITORIAL.

THE AMERICAN MEDICAL ASSOCIATION.

AS our readers are aware, in a very short time the American Medical Association meets. Already have editorial quills been busy with the coming event, and the annual rain upon the much-abused Association has set in. The most unkindest cut of all is, to our thinking, a recent editorial in one of our New York cotemporaries, in which the virtuous, though sleepy, critic upbraids the Association for not doing last year that which it did do, and acutely argues for the adoption at the next session of certain organic laws, which were really acted on and passed at the St. Louis meeting. We are rejoiced to find that Philadelphia does not have a monopoly of Rip Van Winkles, and to learn that the whirling metropolitan maelstrom has one corner in which it is possible to enjoy a year's nap.

The journal referred to advises most strongly that a committee be appointed to consider ethical questions. In truth, such a committee was chosen, under the name of "Council," and, as any one can see by looking over the printed Transactions, has met and entered upon the revision of the Code of Ethics.

The relief afforded by this Council, we believe, will be very great. The bad repute of the Association is very largely due to the disputes to which questions of ethics have given rise; indeed, the ethical disorder has been so violent as to threaten dissolution. We trust the remedy will be found sufficient, and that the waste of time in unseemly quarrels will occur no more. This much accom-

plished, does anything more remain to be done, and is it possible to have an association which shall represent the profession of the United States as the British Association does that of Great Britain? Answering the last of these questions first, we are inclined to say "No."

There are certain geographical or physical laws which it seems to us must prevent our Association from attaining the position held by the English body; chief among these is the great extent of the country. In England, let the Association meet almost anywhere, the bulk of the practitioners in the kingdom can go to it by some early morning train, and return the same night; but here, to attend the meetings is a work of days, and sometimes of weeks, and the expense is commensurate with the time consumed. As the Association usually gathers in a fairly busy month, it is unreasonable to expect a large proportion of the most active practitioners to attend its sessions. If the meetings were arranged for August, and were held in some cool northern position, it is possible the attendance would be more representative of the best and busiest section of the profession.

The great size of our country also lessens the attractiveness of our Association to individuals. Sir William this or that goes to the British Medical because he meets those who either call him in consultation, or who he hopes will do so; for all England is his field of practice. Leading practitioners here may find it their interest to attend State medical conventions; but who expects to be sent for in consultation one or two thousand miles? Evidently, the most powerful of motives—self-interest—draws the professional leaders much more strongly in England than it does with us.

Be these things as they may, we are of those who believe in the necessity of some central organization, which shall, as far as possible, represent the profession in the United States, and we intend, therefore, to sustain the American Medical Association as much as we are able. Free discussion is the only way to arrive at the truth, and it behooves every medical man to ask himself, what can be done to build up the central society? The ethical troubles having been settled, the next point evidently is to get as high a class of delegates as possible. To secure this, a motion was offered at the last session, and is to be acted upon at the Detroit meeting, confining representation to such County and District Medical Societies as are recognized by their respective State organizations, and to the army and navy. The chief object of the resolution is to cut off the colleges; and we think this is a good move; yet it appears

that it would be better to allow representation of such educational institutions as really require a three years' course of study. The only school in the country that could claim a position would be Harvard, and the by-law would be a standing protest against the present educational Cheap John system so degrading to the profession. The proposed by-law certainly needs some alteration in language, if, as appears to us, it cuts off such medical bodies as our College of Physicians and the New York Academy of Medicine, to say nothing of pathological and other lesser societies. In Philadelphia a very large proportion of the best men of the profession belong to the College and do not belong to the County Medical Society; so that, as far as concerns this city, the proposed amendment would be simply suicidal.

A matter upon which no action has been taken, that we are aware of, is the Transactions. Of these, twenty-four volumes have been published; and on referring to vol. xxiii. we find the expense for that year was fourteen hundred dollars. Estimating the annual outgo as one thousand, we get a total of twenty-four thousand dollars; spent in what? Simply in disgracing the American profession before the world. A grain of wheat here and there, to be sure, but mainly chaff, chaff, chaff, fit only to be burnt up by unquenchable fire. Let us do away with the Publication Committee altogether—put an end to that body, whose organic type does not rise as high as that of the amphioxus, the lowest of the vertebrata, in that it is not even furnished with a *chorda dorsalis*. The Association may rely upon it that anything worthy of preservation occurring at its sessions will be embalmed by the journals, and that it would be a general relief if the Transactions were confined to the business minutes. A thousand dollars a year, properly expended, would be a godsend to American medical science. What we most need is original investigations. Let two prizes of five hundred dollars each be offered, and *not given unless earned*, and we conceive the result would be most happy. In a recent article upon "Universities: Actual and Ideal," Prof. Huxley says,—

"The other day an emphatic friend of mine committed himself to the opinion that, in England, it is better for a man's worldly prospects to be a drunkard than to be smitten with the divine dipsomania of the original investigator. I am inclined to think he was not far wrong. And be it observed that the question is not whether such a man will be able to make as much out of his abilities as his brother, of like ability, who goes into law, or engineering, or commerce; it is not a question of 'maintaining a due number of saddle-

horses,' as George Eliot somewhere puts it; it is a question of living or starving."

What is true of England is a hundred times more true of America. With as much of talent, we firmly believe, as any other country in the world, we do not accomplish as much of original investigation as a third-rate German town; largely because we systematically starve out, or indifferently freeze with neglect, such talent. To be sure, our lordly association offers the magnificent sum of one hundred dollars as a prize. No wonder it gets prize essays which the medical journals would scarcely print.

We acknowledge that five hundred dollars is too small a sum for the Association to offer as an inducement to original investigation; but let us not despise the day of small things, and as it is probable that at first the country could not produce more than one essay worthy of the prize, a fund might be obtained for increasing the amount.

CORRESPONDENCE.

SLOWTOWN, May 15, 1874.

SIR,—I have read with indignation which I do not care to conceal the recent shameless attack upon the Board of Trustees in this city. Exhausted by the cares and trials of the recent election to which your correspondent alluded, I fled to a distant watering-place to recruit my conscience and my shattered constitution, and there in a reckless morning journal I read the cruel and covert attack upon the body to which I am proud to say I belong.

It is impossible to leave unanswered the charges thus made, and when I assure you that in no instance did more than one hundred and thirty-two persons call upon me for any single candidate, that the board in question contains no grandmothers, that your definition of canvass exists in no dictionary, that the cost of our broken-door bills is provided for by a fund left to the college ages ago by a successful candidate, I trust you will be satisfied.

Perhaps you would like to know how we do really secure a good choice. In the first place, we ascertain who among the candidates lectures best. This we get at by asking their office-students. If a man lectures well, we drop him at once, since it is our policy to train our own professors, and what would be the use of training a man who can already lecture? If, too, we find out that a man has written essays which are valued and quoted in Europe, we put him aside, because it is impossible that any one abroad can know as much about a doctor as we do. In this way we manage to eliminate until we have left some one who having a minimum of past reputation will be sure to work hard in order to acquire a future name. In other words, we go in for expectation. We thus make sure

that our professors shall be active working men, with a reputation to make, not an effete people who have worn out their brains in past efforts. Our men are to be taken on trust, and this is the real meaning of the word,—a trustee being a fellow who elects a fellow whose future is to be taken on trust by other fellows in general.

Yours, etc.,

AN INDIGNANT TRUSTEE.

PHILADELPHIA, May 10, 1874.

DEAR SIR,—Many years ago I had the grim misfortune to become a candidate for a vacant chair in this city, and I beg to assure you that the fun of *Lancet's* letter from Slowtown but faintly describes the reality. Consultations of my friends were held as to every trustee—twenty-four, I think. Who were their clergymen, doctors, creditors, relatives? What church influence reached them? Which of them were believed to listen to advice from their wives, and who were their wives' clergymen? and so on. Who was to be pushed steadily? who not? Who needed somebody else "on him"? I assure you, sir, I look back with loathing on that time; and yet no man who has tried this vile path to preferment can say I overdraw the picture. Some one may answer, Why do physicians resort to such devices? Why not send to the individual trustees a list of the papers you have written, invite them to hear you lecture, or give them the written opinion of half a dozen men of note as M.D.'s concerning capacity to teach? Ask them to write, as they should do, to the dozen or so of men in your own line throughout the country who can say with authority of what force you are in the pursuit you profess to have excelled in, and then leave to them the freedom of a thus instructed choice.

Does any man who reads this presume that such a course would prevail against the grandmothers, church influence, insurance-board associations, this or that railroad? I for my part see no hope save one. The time will come here, as it has come in New York, when the faculties will possess the true power to elect, or at least to nominate; when they will have representation in boards of trustees, and when the alumni will insist upon a like privilege; then may we expect to see mercantile and other considerations which are called "influence" yielding to an honest judging of the candidates by the mode in which they have illustrated their branch, and by the known measure of their capacity to excite their own enthusiasm for its pursuit in those whom they teach.

Faculty representation.

Alumni representation.

These are what we want in our boards of trustees.

My remarks are called out by unwholesome memories of a past canvass, in which I did not get off with triumph, nor has what I have urged been meant to have the least reference to any contest which has taken place of late. The concours and the German system of promotion have alike their faults, but in the con-

cours it is at least impossible for the worst man to be chosen, while under the German plan he who does not justify his elevation to a lower class of chairs is little likely to rise to a higher one. Work of some kind—original productiveness—is there the test, and is, in fact, a good, if not a perfect one.

Yours,

MAT. MED.

PROCEEDINGS OF SOCIETIES.

BIOLOGICAL AND MICROSCOPICAL SECTION OF THE ACADEMY OF NATURAL SCIENCES.

MAY 4, 1874.

DIRECTOR W. S. W. RUSCHENBERGER, M.D., in the chair.

DR. JAMES TYSON exhibited a beautiful case of one hundred microscopical specimens in pathological anatomy, prepared by Dr. Otto Barth, of Leipsic, which he had recently purchased. He remarked that while he had found some of the sections so thick as to be almost worthless, the majority were thin enough to display structure quite satisfactorily with moderate powers such as 200 diameters. The preparations comprised illustrations of a great variety of diseased organs, and among the rest he would call particular attention to those illustrating the pathology of croupous and catarrhal pneumonia, tubercle, and the different forms of renal disease, including the fatty kidney, the granular kidney, interstitial nephritis, and the lardaceous or amyloid kidney, showing casts, apparently of the lardaceous material, blocking up the tubules. This latter example was important on account of its bearing upon the question as to whether lardaceous disease could be diagnosed by the appearance of casts, which he considered quite possible in some instances, although he had himself never been able to obtain the red reaction of iodine in so-called waxy casts, as found in the urine.

Dr. JOSEPH G. RICHARDSON remarked that he had likewise experimented very often upon the reaction of iodine with the tube-casts of amyloid disease of the kidney, and had in three separate cases been able to obtain the characteristic mahogany-red tint with iodine, acting upon the wax-like casts. In two of these cases he had made the diagnosis of amyloid form of nephritic disorder in patients in the Pennsylvania Hospital, and had the correctness of the opinion fully confirmed by subsequent post-mortem examination. In a recent instance he had been fortunate enough to meet with a small waxy cast, enclosed in a larger one of the ordinary pale, granular variety, and found it, of course, an excellent opportunity for demonstrating the dark-red tint of the iodized amyloid material, as contrasted with that pale-yellow hue given to most animal matters by tincture of iodine, and which Dr. Grainger Stewart states he has never seen exceeded in tube-casts.

Dr. TYSON said he had demonstrated to his satisfaction the utility of testing this reaction with alcoholic solution of iodine, which produced a granular precipitate of albumen throughout the fluid, and completely obscured the field of view.

Dr. RICHARDSON desired to correct his statement by adding that he used in these experiments not the tincture of iodine (mentioned inadvertently), but a diluted solution of the liq. iodini comp., U. S. P.

Dr. J. GIBBONS HUNT inquired how these preparations of Dr. Barth were hardened for cutting the sections.

Dr. TYSON said that he had no definite information respecting these particular specimens, but thought from what he knew of German methods of work that they were probably hardened for cutting in solution of bichromate of potassium or of chromic acid. From an accident which had occurred to one of the preparations, resulting in the fracture of its thin glass cover, he was led to conclude that they were cleared with oil of cloves and mounted in balsam.

Dr. HUNT remarked that when he had first seen the preparations of Dr. Barth, he thought they were reasonably respectable; but on closer examination he had found the cellular elements of the structures shrunken and altered, until, like too many of our own mounted specimens, they came to look merely like caricatures of the tissues, from which they were actually prepared by a process of distortion. All the productions of Barth which he had carefully inspected were, he believed, hardened in alcohol, by the imperfect method now used in the Army Medical Museum at Washington. According to his own experiments, structures which have been judiciously hardened in chromic acid solutions, and properly stained, especially with hæmatoxylin, exhibited all their cell-elements with admirable distinctness. So superior were the chromic acid solutions, that he could easily demonstrate in a portion of spinal cord, suitably hardened in Müller's fluid, not only every minute blood-vessel, but even the red blood-corpuscles which they enclosed. In proof of these statements he had brought with him to the meeting a section of kidney hardened as above suggested, and stained with perchloride of tin, which exhibited the cells lining the convoluted tubules, the straight tubules of Henle, and even the delicate cell-layer seated upon the inside of the capsule of the Malpighian bodies, as he had never before seen it demonstrated.

Dr. TYSON quite agreed with Dr. Hunt in his unfavorable estimate of alcohol as a menstruum for hardening tissues, and had, some time since, concluded to abandon its use for that purpose. He wished to know whether Dr. Hunt had not been able to bring into view the endothelium of Bowman's capsule by means of nitrate of silver.

Dr. HUNT replied that the silver method enabled us to demonstrate these cells without much difficulty, but at the same time less satisfactorily than the one he had proposed. If delicate cell-elements are properly stained they can be with ease perfectly preserved in balsam, whilst it is only by superior skill in manipulation that thin sections can be prepared and mounted permanently in glycerin.

Dr. TYSON remarked that when in London, two years since, he had had an opportunity of examining some of the glycerin-preparations of Dr. Lionel Beale, and found them just as figured in his various works. They were not, however, sections, and he could not understand how sections could be made of preparations which had only been treated with glycerin, because in it there is no hardening power, and no chance is afforded for the production of any but teased-out specimens. He desired to ask Dr. Hunt what his experience had been in the preparation of specimens by freezing.

Dr. HUNT replied that the method by freezing did not involve any more trouble, but had the disadvantage of requiring special and, for its most successful practice, somewhat complicated apparatus. He considered it very important that the action of the frigorific mixture should be stopped short at the point of waxy hardening, and not allowed to proceed until the stage of completely brittle congelation is reached.

Dr. RICHARDSON exhibited a specimen of leucocythæmic blood preserved in a perfectly sweet condition by mixture with an equal bulk of hydrate of chloral

solution (3j to f3i), and mentioned as a peculiarity of the case of leucocythæmia the rapidity of increase of the white blood-corpuscles, their ratio to the red disks being as 1 to 2 ten days before the patient's death, whilst after dissolution it was found to be as 2 to 3.

REVIEWS AND BOOK NOTICES.

TREATMENT OF NERVOUS AND RHEUMATIC AFFECTIONS BY STATIC ELECTRICITY. By Dr. A. ARTHIUS. Translated from the French by J. H. ETHERIDGE, M.D., Professor of General Therapeutics, Rush Medical College, Chicago.

Who Dr. Arthius is we do not know, and, if we may judge from the preface of the translator, the American godfather does not appear to have any more definite idea as to the father of his adopted child, for he states that the original treatise was handed him by a friend, who received it from a friend who had been a patient of Dr. Arthius.

If any one of whose truthfulness we had not the clearest knowledge should say to us that he had cured, by a few applications of electricity, epilepsy which had lasted for twelve years, and at the same time restored a right arm that was withered and powerless, we would gaze on him "with a smile that was childlike and bland." Precisely this is what is claimed in the brochure before us; and can we be blamed that the famous witticism which tradition attributes to Dr. Rush has been uppermost in our mind?—"The French lie, and Dr. — relies on them."

The tone of the book seems to us to indicate that it has been written rather for the laity than the profession; rather to be handed to the seeker after health in the office, than for sale to doctors.

There are several points of interest in the book; chief among them is the so-called "fluidique bath," by which is meant placing the patient upon an insulating stool, and connecting him with the prime conductor of an electric machine; of this marvellous bath it is written that "the fluidique bath induces an acceleration of the pulse, and is singularly calmant, eases the respiration, develops animal heat, augments cutaneous transpiration, makes more active the urinary secretion, disperses nervous irritation, gives tone to the whole organism, increases the vital forces, and augments the energy of absorption," which is altogether too glibly said to induce us to believe that it was in any way proven by rigorous experimentation.

The most extraordinary statements are made by Dr. Arthius in regard to the diagnostic use of static electricity. We are told that when a person is charged by means of the electric bath, and the hand or a metallic conductor passed over the body, the moment a diseased part is reached it will at once be perceived by both patient and doctor. Hence we can search for disease as the peasant searches for water with his witch-hazel.

Other marvels are told of in this book of Aladdin's lamp; metals are said to be carried into the system by this wondrous static electricity, and the powers of medicines to be increased manifold times. The entertaining character of the book is not lessened by some ludicrous wordings, for which, it may be, we have to thank the translator. Thus, Dr. Arthius speaks to a patient of electricity generated by piles, and hence of hemorrhoidal nature, we suppose.

In taking leave of the brochure, we cannot but express a wish that what it says were true; as, were it so, new diagnostic and therapeutic powers of great value would be in our grasp.

GLEANINGS FROM OUR EXCHANGES.

SUB-CORACOID DISLOCATION OF THE SHOULDER WITH FRACTURE AND ANCHYLOSIS (*The Irish Hospital Gazette*, May 1, 1874).—At a meeting of the Dublin Pathological Society Prof. Bennett presented the right scapula and humerus, taken from a dissecting-room male subject, which presented some unique features. There was no life history. The muscles situated around the shoulder-joint were wasted, but not in a state of advanced degeneration; therefore the accident which caused the anchylosis could not have occurred very long before death. On examining for the cause of the anchylosis, a sub-coracoid dislocation of the shoulder, with a fracture of the upper part of the humerus, was detected. Of this variety of fracture, said Prof. Bennett, only eight examples were recorded by Malgaigne. An example was also recorded in Anger's *Traité Iconographique des Maladies Chirurgicales*, in which, however, the right humerus was depicted with the left scapula; this specimen might consequently be legitimately excluded as being a made-up case. The fracture in these instances passed through the anatomical neck of the bone, then through the bicipital groove, and involved two or three inches of the shaft of the humerus. It is an oblique fracture, and generally spares the great tuberosity. In the present specimen the tendon of the biceps was included in the callus of union. Prof. Bennett observed that his case was unique, as he was not aware of any other of this form of dislocation and fracture of the humerus that had been followed by anchylosis.

SIMULATED DISEASE (*The Lancet*, April 4, 1874).—Messrs. Davidson and Puzey report a case of feigned hemiplegia, and conclude that, in investigating the symptoms of motor paralysis, the following tests are available:

1. The well-known appearance of dragging in a paralyzed leg; 2, the test suggested by Dr. Hughlings Jackson:—in real hemiplegia the paralyzed arm falls forward when the patient stoops; in a case of feigning, the arm will probably be retained by the side; 3, the condition of the muscles in long-continued paralysis:—where paralysis has continued for several months in a limb, we should expect that the muscles would both diminish in size and lose their electro-contractility; 4, in cases where the patient says that he has been confined to bed for many weeks, the condition of the soles of his feet may be taken as a test of the correctness of this statement. A scaly and cracked condition of the cuticle of the soles will, according to our experience, be invariably found in adults (at least in the lower ranks of life) who have been confined to bed for a considerable time.

In all cases of suspected feigning of disease we should endeavor to ascertain whether the patient has had opportunities of becoming acquainted with the symptoms of the disease supposed to be simulated.

ELEPHANTIASIS GRÆCORUM.—Dr. Leisrink relates a case of this kind in the *Deutsche Zeit.*, vol. iv., in which he tied the femoral artery just below Poupart's ligament. The lady, 51 years old, had suffered twenty-five years before from repeated attacks of erysipelas in the right leg, and the latter measured twenty inches round the calf, whilst the sound leg measured only seven. She was considerably weakened by oozing of serum, and four days after the operation the size was reduced to twelve inches. It rose a little afterwards, and, although the patient was not actually cured, the disease was certainly arrested and modified. Compression of the artery has been tried in this and other countries with varying success, but amputation has almost always proved fatal.—*London Lancet*.

ACUTE SOFTENING OF SPINAL CORD (*The Lancet*, March 28, 1874).—A girl, æt. 11, who had always enjoyed perfect health, was seized with shiverings and pain in the limbs, and, four days later, suddenly lost the use of her legs. Paralysis of motion was complete; sensation was diminished in the right leg, and was absent in the left. There was no reflex action. The condition grew worse; the arms became involved, and at the end of a week she died. At the post-mortem examination a quantity of serous fluid was found in the spinal arachnoid sac, and the cervical portion of the cord was soft and disintegrated.

GUNSHOT WOUND OF HEART (*Indian Medical Gazette*, 1874).—A tiger was shot with a rifle from a distance of about four feet. He ran a distance of about one hundred and forty feet, and then died. It was found that the ball had struck him about an inch to the right of the spine, passed through the right lung, struck the heart in the septum ventriculare, below the right auricle, and torn a huge hole through the right ventricle, besides opening the left into the right. It had then grazed the left lung and emerged below, an inch and a half to the left of the mesial line.

SUDDEN DEATH AFTER WOUND OF A FINGER (*Gazette des Hôpitaux—Clinic*, March 7, 1874).—A man was wounded at the extremity of an ungual phalanx by a hatchet. The wound was dressed by a simple bandage. Progress was favorable. The wound began to cicatrize, with very little suppuration. There was no fever, or even malaise, and the patient was soon able to work about the hospital. Suddenly, as he rose from his bed one day, he fell to the floor dead. A careful autopsy revealed evidence of intense bronchitis, and eight or ten abscesses in the liver.

TREATMENT OF TÆNIA (*The Medical Press and Circular*, March 25, 1874).—Dr. Bill recommends the use of phenic acid in the treatment of this worm, and gives an illustrated case where koussou, male fern, turpentine, and other remedies were useless. He gave six grains of phenic acid in half a pint of water four times a day; in two days segments of the worm were discharged. He then gave the acid in shape of pill, and on the third day the remainder and head of the animal were discharged.

TREATMENT OF ALCOHOLISM BY NUX VOMICA (*Irish Hospital Gazette*, May 1, 1874).—Dr. Luton has obtained excellent effects from the use of nux vomica in chronic alcoholism where the evil has not passed into the absolutely degenerative stage of tissue-change. In the tremors, cerebral, gastro-intestinal, and thoracic disorders of alcoholism, he resorts with confidence to the use of extract or tincture of nux vomica in ordinary doses.

TREATMENT OF VARICOSE VEINS (*Dublin Journal of Medical Science*).—"Should the surgeon be urged to meddle with varices, we would suggest, in preference to the use of the Vienna paste, a careful injection of the vein with the perchloride or the subsulphate of iron. More people have probably escaped death from this treatment than from any of the other numerous operative measures invented for the treatment of the varicose condition of veins."

CONGENITAL STRANGULATED HERNIA IN AN INFANT.—Dr. Somerville (*British Medical Journal*, January 3, 1874) reports the case of a child ten days old, who suffered from a congenital strangulated hernia. A spontaneous cure took place.

A USEFUL extemporaneous splint for fractured limbs is suggested by M. Noisette, consisting simply of gutter-shaped pieces of zinc or tin plate, perforated if possible, for the sake of reducing its weight.

MISCELLANY.

HYDROPHOBIA.—The police authorities in Vienna call attention to the fact that numerous cases of hydrophobia have recently occurred, and that many persons have been bitten by dogs which were actually mad or supposed to be, and have also issued circulars of advice to the owners of dogs. They say that the possessors of these animals should take good care of them, and watch attentively for any symptoms of disease which may indicate the beginning of rabies. Unusual irritability and loss of appetite are set down as sufficient to cause anxiety, even when the animal still continues to drink water. Dogs presenting these symptoms are to be secluded and watched, or, with due precautions, handed over to the authorities of the Imperial Institution for the Care of Diseased Brutes. Dogs are not to be taken into public conveyances or brought into hotels, coffee-houses, and like places of public resort, and when they are taken upon the street, a leash must be used. Violation of these rules subjects the animals to the risk of capture and death from the officials charged with this duty.

It is generally imagined by the public that the antipathy of the profession to quackery originates in a feeling of self-interest, and that quacks take the bread from medical men. We more than doubt if this is true. If the faculty lose and quackery gains a particular case, that is amply compensated by the ill health which quackery produces in thousands, and the disposition to fly to physic which it generates in millions. On the whole, the profession probably gains by quackery—certainly its loss must be very trivial. A feeling of self-interest can sway the profession very little either way, and its antipathy to quackery must be sought on other grounds.

We should say that our aversion to quackery takes its rise in a natural feeling of disgust at a barefaced system of humbug. Unless men's minds are warped by the strongest motives towards wrong, they revolt at witnessing it. We see a knot of rascals fleecing and injuring the public by the most dishonest and scandalous arts. We naturally evince contempt and indignation towards the miscreants.—*Canada Lancet*.

PROSECUTION FOR SYPHILITIC VACCINATION.—At a recent sitting of the criminal tribunal of Hamburg, a public vaccinator, Dr. S., was found guilty of causing injury to the body by neglect, through vaccinating from a syphilitic child, and was sentenced to a month's imprisonment. The facts of the case were briefly these: On August 21 of last year, a male child a year and a half old was brought to the vaccination station of which the defendant had charge, and, under the directions of the defendant, whose duty it was to choose the children from whom vaccination should be performed, was vaccinated from a child a year old, who had syphilis, and itself became affected with the disease.—*British Medical Journal*.

WE are indebted to Dr. William W. Keen for an opportunity to read some letters from Dr. John C. Berry, Medical Missionary in Japan. The government has permitted the opening of a dissecting-room under the charge of Dr. Berry, and the Japanese native doctors manifest the greatest enthusiasm in studying practical anatomy. Dr. Keen's edition of Heath's Dissector's Manual is being translated into Japanese, and the proofs of copper (?) engravings made by native artists are the handsomest anatomical cuts we have ever seen.

THE number of students attending the lectures of the Vienna University during the past semester was 3813, of which number 1109 were students of medicine. In the total number, 3813, there is an increase of 373 over the number of students registered for the summer session of 1873.

A CORRESPONDENT in the *London Pharmaceutical Journal* wants to know what becomes of the enormous quantity of *Cocculus Indicus* that is imported. From his statement, it appears there are at present in stock in London some twelve thousand pounds of this poisonous drug, with no visible means for its consumption.

DR. A. E. SANSOM has been appointed Assistant-Physician to the London Hospital.

NOTES AND QUERIES.

MULTIPLE IMPREGNATION.

TO THE EDITOR OF THE PHILADELPHIA MEDICAL TIMES:

DEAR SIR,—The article entitled "Multiple Impregnation," published in the *Philadelphia Medical Times* of the 18th ult., has recalled a similar instance. While in Arizona, I had in my possession a white slut (pointer), which was covered by a dog of her own color and species, and the next day by a black, long-haired cur. In due time she gave birth to seven white pups, and in thirty hours to another which had coarse black hair and resembled the cur in every particular. I watched the growth of these pups with great interest, and found that while the white pups learned to hunt birds intuitively, it was almost impossible to teach the black one.

Very respectfully,
BLUE PILL

SAN FRANCISCO, CAL.

INAUGURATION OF THE HOSPITAL OF THE UNIVERSITY OF PENNSYLVANIA.

The inauguration of the splendid new hospital which has been erected in connection with the Medical Department of the University in West Philadelphia will take place on Thursday, June 4, at three o'clock P.M. Governor Hartranft will preside at the ceremony, and Hon. William A. Wallace will deliver an address. All medical alumni of the University are invited to attend.

OFFICIAL LIST

OF CHANGES OF STATIONS AND DUTIES OF OFFICERS OF THE MEDICAL DEPARTMENT U.S. ARMY, FROM MAY 19 TO MAY 25, 1874, INCLUSIVE.

BILL, J. H., SURGEON.—Relieved from duty at David's Island, N. Y. H., and assigned to duty as Post-Surgeon at Fort Wood, N. Y. H. S. O. 94, Military Division of the Atlantic, May 9, 1874.

FRYER, B. E., SURGEON.—Relieved from duty at Fort Wood, N. Y. H., and assigned to duty at Fort Wadsworth, N. Y. H. S. O. 94, c. s., Military Division of the Atlantic.

TILTON, H. R., ASSISTANT-SURGEON.—Assigned to duty at Fort Sully, Dakota Territory. S. O. 94, Department of Dakota, May 12, 1874.

MIDDLETON, P., ASSISTANT-SURGEON.—Assigned to duty at Fort Duncan, Texas. S. O. 72, Department of Texas, May 12, 1874.